

shown in the Memoir, involved in and given immediately by these forms. It is also shown that the formulæ are greatly simplified in the case *e. g.* of tangents drawn to a conic from a point in a conic having double contact with the first-mentioned conic, and that in this case they lead to the linear automorphic transformation of the ternary quadric. The Memoir concludes with some formulæ relating to the case of two conics, which, however, is treated of in only a cursory manner.

May 22, 1862.

Major-General SABINE, President, in the Chair.

The following Gentlemen were proposed by the Council for Election as Foreign Members, and it was announced that they would be balloted for at the next Ordinary Meeting of the Society, viz.:—

César Mansuete Despretz, of Paris.

Franz Ernst Neumann, of Königsberg.

Ernst Heinrich Weber, of Leipsic.

The following communications were read:—

- I. “Letter to the President from Mr. WILLIAM LASSELL, F.R.S., dated Malta, May 13, 1862, giving an account of Observations made with his large Equatorial Telescope.” Received May 22, 1862.

9 Piazza Stierna, Malta,
May 13, 1862.

DEAR GENERAL SABINE,—I have ventured to think that a word of my proceedings may be acceptable to you, though I have been much more tardy in getting into observing order than I had expected. It is indeed only now that I am able to make observations without finding some one part or other of my apparatus capable of improvement. At length, however, I find my hopes exceeded in the perfection, precision, and facility with which my colossal equatorial is directed and carried on: the driving motion is indeed as perfect and uniform, I believe, as that of any telescope with which I am acquainted. For the luxury of observing two assistants are necessary,

when the observer has really nothing to do but keep his eye at the telescope.

We have passed through what was called, for this climate, an unusually cloudy winter, and it is only now that the weather is becoming settled for the summer, and only now that I may be said to be entering upon regular work. I have indeed carefully observed some of Lord Rosse's nebulae, and in at least two or three instances can fully confirm the spiral character attributed to them by his Lordship,—not, I think, when the objects are well seen, to be overlooked, even when the mind is not previously possessed with the idea. I am making careful drawings of these nebulae as I see them, some of which closely resemble Lord Rosse's, while others are so different as to suggest (with the fact of the lost nebula in our remembrance) the idea of a real change of form. With new objects, however, of so much delicacy it is necessary to survey them again and again, under different circumstances, in order to arrive at a trustworthy conclusion.

One object, on which I scarcely intended to bestow any attention, has fascinated me greatly—I allude to the moon, in which I see minute details with a hardness and sharpness and reality I have never seen before. My opportunities of scrutiny have, however, been fewer than might have been supposed, from my having frequently been engaged in showing this very popular object to many visitors. Yet, notwithstanding that I have thus been able to see more into the moon than ever before—so much so that I believe, if a carpet the size of Lincoln's Inn Fields were laid down upon its surface, I should be able to tell whether it was round or square,—I see nothing more than a repetition of the same volcanic texture—the same cold, crude, silent and desolate character which smaller telescopes usually exhibit.

Saturn is just now an object of much less physical beauty than when I was here in 1852. I observed, however, on the 15th of April, the passage of Titan on to the disk of the planet, near the northern limb, a phenomenon which of course can only be observed in or near the present position of the ring, and therefore interesting from its rarity.

With respect to the climate, I have not yet used this telescope in its most favourable season. In 1852 I may be said to have gauged the purity of the sky during the Indian summer with an aperture of two feet; now I have been gauging it during a less favourable season

with a four-foot aperture; and therefore it is no wonder if I find nights of the requisite degree of tranquillity somewhat more rare. Yet I find my own physical strength insufficient to allow me to use up half the quantity of available sky, and my next want will probably be some efficient and energetic assistance in the duty of observing.

To General Sabine,

President of the Royal Society, &c.

I remain, &c.,

WM. LASSELL.

II. "On the Theory of the Motion of Glaciers." By WILLIAM HOPKINS, Esq., F.R.S. Received April 14, 1862.

(Abstract.)

Almost all the numerous discussions which have taken place during the last twenty years respecting our theories of glacial motion have had for their object the assertion of some particular view, rather than the establishment of a complete and sufficient theory founded on well-defined hypotheses and unequivocal definitions, together with a careful comparison of the results of accurate theoretical investigation with those of direct observation. Each of these views has been regarded, improperly, in the author's opinion, as a *Theory of Glacial Motion*. The Expansion Theory ignored the Sliding Theory, though they were capable of being combined; the latter theory was equally ignored by the Viscous Theory, in which, moreover, instead of the definitions of terms being clear and determinate, no definition of *viscosity* was ever given, though that term designated the fundamental property on which the views advocated by this theory depended. Again, the Regelation Theory is not properly a theory of the motion of glaciers, but a beautiful demonstration of a property of ice, entirely new to us, on which certain peculiarities of the motions of glaciers depend.

When we shall have obtained a *Theory of the Motion of Glaciers* which shall command the general assent of philosophers, no qualifying epithet will be required for the word *theory*; it would indeed be inappropriate, as seeming to indicate the continued recognition of some rival theory. If, for instance, it should be hereafter admitted that the sliding of a glacier over its bed and the property of regelation in ice are equally necessary, and, when combined, perfectly